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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Effects of upwelling duration and phytoplankton growth regime on dissolved oxygen levels in an idealized Iberian Peninsula upwelling system" *by* João H. Bettencourt et al.

Anonymous Referee #2

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In the manuscript, a configuration of the CROCO model is discussed, consisting of a simplified version of the study region, using a periodic channel, with periodic winds favourable to the coastal upwelling, disturbed to originate mesoscale structures (eddies and filaments). In particular, emphasis is placed on studying the response of the system to explain the physical mechanisms of the variability (and offshore export) of dissolved oxygen and the phytoplankton growth. The role of oxygenated water export associated to frontal turbulence is identified as the main dissolved oxygen sink near the coast. It is argued that primary production and exchange with the atmosphere rep-

resent mechanisms that compensate the dissolved oxygen sink for the open ocean. While I generally agree with the comments of Referee #1, in terms of the quality of the manuscript, and its relevance of publication, there are a number of points that I will refer to, which should be addressed before the final publication. On the one hand, the configuration used is highly idealized, and the practical application to the Iberian system is not straightforward. Provided the simplifications that were done related the idealized configuration, and the theoretical approach, (quite difficult to read to oceanographers not specialized in theoretical oceanography) I have some doubts about the practical applicability of the results, and the interest that it may have for the oceanographic community of the region, devoted to the study of biogeochemical processes, (constituted mainly by chemical oceanographers).

Summarizing, the points below should be addressed:

1 - Introduction: Paragraph 1. The Iberian region is not an example of a region of marine hypoxia, and that should be referred to in the manuscript. References to deoxygenation (line 40) do not make much sense in the scope of this manuscript. 2 - The authors barely refer to the observational literature related to the oxygen cycle in the Iberian region, limiting themselves to cite Rossi's articles (2010, 2013), which consist of a single campaign, not particularly devoted to that subject. There is some observational literature including the oxygen cycle that I think should be cited, as it is relevant. (Hint: Alvarez-Salgado, X.A, Perez, F.F, Castro, C.G. , all of them in IIM-CSIC-Vigo, Spain) On the other hand there are oceanographic processes not solved by the model that influences the oxygen cycle, related to processes in the benthic layers, and on remineralization processes that influence the oxygen cycle, and that are not referred to. 3 - Lines 62-64 need a reference that support those statement. 4 - Is the implementation of the Biogeochemical (BGQ) module done in CROCO model, or was implemented from scratch?. This is not clear in the ms. In that case, justify why none of the other BGQ modules was not used. 4- Is not clear the interaction with the atmosphere in the BGQ module, concerning O2 atmosphere - ocean exchange. 5- Laplacian mixing

along sigma surfaces tends to generate spurious currents. Why you did not use (rotated) mixing along Geopotential? 6 - Is the wind only alongshore?, please specify.(l 109). 7 - It should be clearer to the reader why nutrients are not introduced specifically, and what is the advantage of doing it in an O2PZ model instead of O2NPZ? 8 - The 12m/s wind pulses although exist in particular years (like the MOUTON campaign) are too high and unrealistic for the Western Iberia region. Justify better why such high values. Some sensitivity studies was done to this value? 9- I don't understand why in the initial vertical profile, (Fig 3a) the 02 drops to zero when in reality the O2 at 300m is closer to 230 mmol O / m3. The range of values in the region above central waters is 220-290 mmol O / m3. 10 - Claim that figure 4 represents a validation seems too optimistic to me. I can't understand what the red dot represents in the figures. 11 - Using logarithms in Chla completely distorts the comparison, because the observations are showing values of 0-10 mg Chla.m-3 between isopycn 26-27 as the model rarely goes beyond 1 (at isopycn 27). Similar comment arises for O2. If you claim those results are validated, then, any result is validated. If you insist in keeping this part as it is, I suggest that you remove the word validation, perhaps use 'qualitative comparison'. 12-Figure 5, I can understand the effect of upwelling at x= 40km that is related to shelf upwelling, but how do you interpret the dome at 60-70km (approximately)? To use the references (l 200-204) to validate the averages of O2 seems to me to be an over-optimistic approach. Again you can say that it is a simplified and idealized model, but do not try to convince the reader that it is validated, because readers with observational background would hardly understand it. 13- Lines 210 - 215 seem more like a discussion, and their presence is not understood there.

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